contains at p. 17 my letter on Dr Barry's discoveries (47).

them (Dr Kilgour's) the pylorus was nearly impervious. There were too few facts to allow of secure conclusions being formed, but Dr Gairdner was inclined to believe that it would be found that fæcal vomiting was to be looked for chiefly in those cases in which the pyloric opening was free, and in which, therefore, the stomach was not overloaded with its own proper contents; while vomiting of food was to be expected in the opposite class of cases. Where fæcal vomiting did not occur, the diagnosis of a fistulous communication like that which existed in those cases must always be obscure. The diminished tendency to vomit food, and the rapid emaciation which might be expected to ensue, were not characteristic; because the ordinary course of cancerous stricture of the pylorus did not in these respects differ very materially from the condition in question. If, indeed, a marked and sudden alleviation of the vomiting occurred, simultaneously with a great reduction in the size of the stomach on percussion, and with the dejection of alimentary matters half digested, while the appetite continued strong and the patient was yet not nourished, a diagnosis might be arrived at.

ON THE DIFFUSION OF CHOLERA IN THE REMOTE DISTRICTS OF SCOTLAND. BY W. T. GAIRDNER, M.D.

Dr W. T. Gairdner made a verbal communication on the results of some inquiries in which he had been engaged on this subject. He found that the epidemic of 1832 had passed with great rapidity to the extreme north, being apparently propagated along the fishing villages of the eastern coast as far as Caithness-shire. Aberdeen and Inverness had both been involved in the epidemic; and the latter place had been again attacked, two years after, in 1834, when the greater part of the eastern coast was entirely exempt. Dr Gairdner suspected that the disease had on this occasion been carried along the Caledonian Canal, possibly from Glasgow or the neighbourhood, but he had not been able to make out distinctly the channel of communication. On the west coast, with the exception of a few cases at Stornoway, and possibly some in the island of Skye, he had reason to believe that no cholera had existed north of the Sound of Mull, nor in any of the Hebrides, with the exceptions just mentioned. He was still in want of information as to the periods of invasion, etc., of the disease on the south side of the Moray Frith, and at a few stations, such as Peterhead and Fraserburgh on the east coast. As regarded the progress of cholera in the interior, and particularly along the line of the Caledonian Canal, he was also imperfectly informed, although he had made attempts to get information both from medical men and clergymen. Dr Gairdner intimated his intention to publish shortly the details in his possession, and said that he would feel greatly obliged by being made the medium of any important information on the points indicated, whether as regarded the first or any of the subsequent epidemics.

The *President* intimated that this was the last meeting of the season; and after noticing in detail the various papers which had been communicated to the Society, with a few appropriate remarks he brought the session to a close.

MEMOIR OF THE LATE MARTIN BARRY, M.D., F.R.SS. L. AND E.

MARTIN BARRY was born at Fratton, in Hampshire, on the 28th March 1802. His father lived here in retirement, but had property embarked in a mercantile concern in Nova Scotia. This was conducted by a brother, who resided there, as acting partner, their vessels trading to the neighbouring colonies, New Brunswick, Newfoundland, West India Islands, and to the United States. It was originally intended to qualify Martin for mercantile pursuits. With this object he received a liberal education, and, at his father's death, was placed for a term of years under the care of a judicious relation in America. At the expiration of

this period, he fulfilled temporary engagements with several eminent firms, in order to increase his experience in commercial transactions. He never engaged in business on his own account, but preferred the medical profession, in order to gratify a love for science, and to apply his mental acquirements more fully in unison with his taste than a commercial life would allow. After thus being trained to business, he thereafter studied in the medical schools of London, Edinburgh, Paris, Erlangen, and Berlin. He became a member of the Royal College of Surgeons in Edinburgh, and obtained his degree of M.D. there in 1833, was a member of several societies, such as the Wernerian, Royal Medical and Physical, and eventually of the Royal Societies of London and Edinburgh.

Soon after graduating he went to Germany, and, as a pupil of the celebrated Tiedemann, at Heidelberg, was closely engaged for about a year in minute anatomical and physiological investigations. At this time, as also during his Edinburgh studentship, he was well known for accuracy in dissection, and skill in making anatomical preparations.

Dr Barry was an enthusiastic lover of natural scenery, and devoted much time to botanical and geological pursuits. He annually visited some of the mountain and lake districts of Scotland during the college vacations; and after concluding his studies at Heidelberg in 1834, spent part of the autumn of that year in travelling much on foot through Switzerland. In the course of these wanderings he arrived at Chamouni on September 15th, and, apparently without any preconcerted intention of attempting such a feat, consulted guides as to the practicability of ascending Mont Blanc. It was then later in the season than any successful ascent had been hitherto accomplished, and much hesitation as to making the attempt was expressed by those he consulted. However, as four years had elapsed since the last ascent was made (by Captain Wilbraham), true also to the impulse of his then great physical energy and ardent temperament, Dr Barry resolved upon the enterprise, and accomplished it with safety to all concerned. His was the sixteenth ascent that had been made; it occupied three days in going from and returning to Chamouni; he was rewarded by a magnificent view from the summit, and by weather so remarkably fine, "that during the whole time," says Dr Barry, "I did not see a single cloud." A short account of this journey was published in the "Edinburgh New Phil. Journal," vol. xviii. In March 1836, he gave (and eventually published), two public lectures, in Edinburgh, on the same subject; their proceeds were presented to the Royal Infirmary; and his excellent model of Mont Blanc, by which the lectures were illustrated, was presented to the University Museum. This ascent gained him the acquaintance of many distinguished persons. Among these was Baron Humboldt, who personally requested Dr Barry to translate from the German an account of his (the Baron's) "Two Attempts to Ascend Chimborazo." This translation appeared in the "Edinburgh New Phil. Journal," 1837.

On returning from the continent, and residing for a time in Edinburgh, in order to extend his medical knowledge by further attendance at the Royal Infirmary, Dr Barry was an occasional and ever welcome visitor in various private families he had known during previous years. Some of these were connected with the religious body (the Society of Friends) to which he belonged. By them he was valued for kind gentleness of heart, refinement of manners, extensive information, and a high tone of moral and religious principle. He naturally possessed a sensitive temperament, which was most obviously shown, when any opinions peculiar to himself were called in question or undervalued. This trait characterized him through life; and owing to the tenacity with which he adhered to opinions once formed, some have considered him dogmatical, or even pedantic. But we must remember, that although his once ardent mental constitution led him to theorize, and even to *believe*, where many would have waited in doubt, it was a conscientious honesty of purpose which taught him to avow and defend, for he could not conceal what he really thought to be true. Having little relish for mere amusement, and possessing something allied to a fixed distaste for convivial company, however intellectual, he never mixed

largely in general society. Being, however, a good classical scholar, conversant with the literature of his own country, as well as with that of France and Germany, familiar with physiological knowledge, full of ardour for its extension, and possessed of great skill in using the artist's pencil, he devoted almost all that time to study which many men with other tastes would have given to what is called pleasure.

Almost the only evidence respecting the time when Dr Barry turned his special attention to embryological pursuits, is to be inferred from his writings alone; he has left scarcely any other materials from which we can collect and record the progress of his scientific life. His letters seldom contain much reference to the manner in which his time was occupied, for some intimate friends with whom he most frequently corresponded were unconnected with the medical profession, or had little relation with the scientific world. He never married; and, having no permanent place of residence, spent the last twenty years of his life in different parts of England, Scotland, and Germany. Edinburgh was a favourite place of abode, and at intervals, varying from a few weeks to a year or two, ever since his studentship, he has continued to visit this city, and remained during periods varying from a few days to upwards of eighteen months. His unfailing friends here were John Wigham, Esq., and his excellent lady, of Salisbury Road. With Mrs Wigham he corresponded more frequently than with any one else, except his own relations, and from her we have ever been able to hear of Dr Barry when he was absent from Edinburgh.

He appears to have commenced making himself familiar with the literature of embryology in 1835; and apparently about this time he was again on the continent, engaged with Professors Schwann and Rudolph Wagner in histological observations. Few men, indeed, have had more frequent personal intercourse with eminent scientific men abroad than Dr Barry. During the two successive years he closely engaged in original researches. The results of these, entitled, "Researches in Embryology," appeared in two series, in the *Philosophical Transactions* for 1838 and 1839, and were rewarded by the Royal Society's medal. His habits of intense study, and almost continuous use of the microscope, not by day only at this time, materially injured his eye-sight, and threw him into a state of seriously impaired health, which continued for several years. On somewhat recovering from this illness, his peculiar sensitiveness of temperament was more apparent than before, and became to him a source of unavoidable inconvenience during the after part of his life. We are sure that, if some who once opposed his views on various subjects were only aware of the pain which they occasioned to this devoted and sensitive-minded man of science, their objections would have been stated with far less asperity. Notwithstanding frequent returns of ill health, Dr Barry eventually resumed the histological observations into which he had been originally invited by the embryological researches. His subsequent papers appeared as follows:—"Researches in Embryology, Third Series; a Contribution to the Physiology of Cells" (1840); "On the Corpuscles of the Blood" (1840); "Supplementary Note on the Formation of the Chorion" (1841); "On the Chorda Dorsalis" (1841.) "On the Corpuscles of the Blood," Parts II. and III. (1841.) (All the above papers were published in the *Philosophical Transactions*.) "On the Corpuscles of the Blood" (*Phil. Mag.*, 1841); "On Fibre" (*Phil. Trans.*, 1842); "Spermatozoa found *within* the Ovum" (*Phil. Trans.*, 1842-43); "Facts Relating to the Corpusles of Mammiferous Blood" (*Phil. Mag.*, 1843); "The Cells in the Ovum compared with the Corpuscles of the Blood—On the Difference in Size of the Blood Corpuscles in Different Animals" (*Phil. Mag.*, 1843); "On Fissiparous Generation" (*Edin. New Phil. Jour.*, 1843) "On the Nucleus of the Animal and Vegetable Cell" (*Edin. New Phil. Jour.*, 1847.) "The last paper in this list" (says Dr Barry) "was written mainly for the purpose of bringing together, into the compass of a few pages, observations made by the author years before, and unfortunately so scattered through long memoirs, that they were to many physiologists unknown, and by others greatly misunderstood."

In the year 1843 he delivered a series of physiological lectures at St Thomas' Hospital, London. In reference to these, and to some of Dr Barry's writings, Professor Owen says—"I have equally profited by the well delivered, well arranged, and original lectures which Dr Barry delivered in 1843. . . . . To an extensive and masterly group of the rich results of physiological research, due to the labours of physiologists at home, and more especially over the continent, Dr Barry adds the rare power of wielding his vast resources in the direction of fresh conquests in science; and I now desire to repeat, what I have expressed on a former occasion, my conviction, viz., that the most important anatomical and physiological discoveries that have been made in this country by the use of the microscope during the present century, are those which have been communicated by Dr Barry to the Royal Society, which have been published in the *Philosophical Transactions;* and have received the honourable mark of the estimation in which they are held by that learned body, by the award to the author of the Royal Medal."—(See testimonials in favour of Dr Barry, when Candidate for the chair of Institutes of Medicine in the University of Edinburgh.)

In the year 1844, Dr Barry became connected, as house surgeon, with the newly established Royal Maternity Hospital in Edinburgh. Here, as might be expected from his industry, zeal, kind-heartedness, and partiality for obstetric practice, he proved himself a most valuable officer. Professor Simpson, in noticing his services at this institution, speaks of him as "our invaluable house surgeon, and a gentlemen to whose talents, and zeal, and humanity, the hospital is deeply indebted for its prosperity." (See Simpson's Obstetric works (p. 454), edited by Drs Priestley and Storer.) Dr Matthews Duncan, who entertains an affectionate remembrance of Dr Barry as a man, and has the highest opinion of his abilities as an accoucheur, informs us that "at that time Dr Barry's name was like a sweet odour to the poor, distressed, parturient females in Edinburgh, specially in the Cowgate, High Street, and Canongate. They used to call him '*Barry Martin*,' indeed generally so; and I am aware that his name is extensively remembered in these places. He reared up diligent students, which is not always the case, from the harassing nature of the midnight work of a midwifery pupil. During his residence in the Maternity, he made, with the greatest care, personally, with his own hand, observations as to the position of the child's head at the brim of the pelvis in the commencement of labour, and afterwards as to its position whilst passing through the pelvis. These observations were of the highest degree of reliability, and were undertaken to test Naegele's important views on these subjects. They corroborated them, and form, in my opinion, the best observations besides Naegele's, at least in this country. He also recorded some valuable cases—of resuscitation of a child, after opium poisoning, by the use of Kemp's battery; on galvanism in exciting uterine contractions; and on vaginal hæmorrhage from laceration. Most of the above mentioned observations are favourably noticed and acknowledged by Professor Simpson in various of his papers relating to these subjects."

There is abundant evidence that, as was the case with everything he studied, Dr Barry distinguished himself in midwifery practice, and we incline to believe that his short residence at the Maternity was not only greatly blessed to the poor, but was more fruitful in useful results to his students, and to science, than is generally admitted or known. When he left the Maternity Hospital, says Dr M. Duncan, "He paid me a compliment which I valued then and still remember with pleasure, in asking me to be his successor, a wish of his which I had the honour of fulfilling." Dr Barry resigned his office in the Maternity on account of another relapse into ill health, induced, as was then considered, by extreme devotion to the duties of his office, and those collateral scientific inquiries which his own active mind and daily avocations suggested. He retired to the island of Arran, a place he greatly enjoyed in consequence of its scenery, and the ample means it afforded in gratifying his taste for geological and botanical pursuits. Here he soon found that vaccination, as a preventive of

small-pox, had been extensively neglected among old and young. Faithful to the promptings of his benevolent heart, and at a sacrifice of much time and personal convenience, he vaccinated most children on the island, and afterwards extended his philanthropic mission in the same way to islands adjoining, as well as to such parts of the nearest Highlands as he found not fully protected by Jenner's discovery. During his residence in Arran, and doubtless from its relation with his favourite study, he commenced the formation of an egg museum. This he afterwards extended, until he had at least one egg of *every British bird*.

Owing to continued ill health, he seems to have done but little in the way of writing or original investigation during the year 1846. In 1847, he published in the *Edinburgh New Philosophical Journal* a paper, previously read at the Wernerian Society, "On the Nucleus of the Animal and Vegetable Cell." Soon after this period, he again went to reside in Germany, and was there in the beginning of 1848, when the chair of the Institutes of Medicine, in the Edinburgh University, became vacant in consequence of its previous eminent occupant, Professor Allen Thomson, having succeeded to the Anatomical chair in the Glasgow University. At the instigation of several scientific acquaintances and personal friends, Dr Barry became a candidate, and produced testimonials of a very high order in proof of his fitness for the professorship. As is well known, Dr Bennett was elected to the chair, and has since continued to discharge its duties with a zeal and ability which have proved that the patrons made a most fortunate appointment. Dr Barry retired before the day of election, on the alleged ground of his entertaining religious scruples against subscribing to the tests then in force. On this subject we quote his own words, extracted from a letter of resignation addressed to the patrons, and dated Edinburgh, 10th July 1848; "Refraining from any remark on the application of the 'Tests' in question in the case of a chair of this kind, quite unconnected with Theology, I have now only to say, that my religious creed (as a member of the Society of Friends), although in harmony with all the essential doctrines of Christianity, is one that would not admit of my subscribing these 'Tests;' and that as the experience of my friends, who have since done me the kindness to wait upon you, accords with my own, that the 'Tests' are an insurmountable obstacle to my being elected, I beg leave, on these grounds alone, to withdraw my application for the appointment."

From 1849 to 1853, he again resided much on the continent, especially in Prague, Breslau, and Giessen; in all these places he was on intimate terms with men eminent in science and art. At this time, and ever after, he suffered greatly from a severe neuralgic affection, which, during certain cold and damp states of the atmosphere, involved almost every part of the body, but at other times was more especially confined to the head, neck, and arms. At the commencement of this continental sojourn, he renewed his investigations concerning muscular tissue, and, when at Breslau, had much intercourse with Purkinje on this and other points involved in his views regarding "*Fibre*." These inquiries resulted in the production of his well known paper in *Müller's Archiv* for 1850, and an extended translated abstract of the same in the *New Philosophical Journal* for August 1852. We find also, by a letter to an Edinburgh friend, written at Giessen on the last day of December 1851, that he was then engaged in studies of another kind, intending, as we understood, to turn them to account in micro-chemical investigations. He says, "I have rarely, if ever, had before me a subject so continually, and so intensely laying hold of all my thoughts, as that to which they have been directed for the last six weeks. . . . It is with *chemistry*, in the far-famed laboratory of Professor (now Baron) Liebig."

In 1852, he revisited Scotland, and resided occasionally in Arran, Rothesay, and Edinburgh. We saw him here for the last time in the summer of 1853. He was greatly emaciated, and suffered from neuralgic pain so continuously that he could not long maintain the same posture, and was often deprived of sleep for

nights in succession. Notwithstanding all this, however, and although his mental and physical prostration were often very great in consequence of prolonged suffering, yet he entered freely into conversation and discussion regarding his favourite objects of study. Almost the only thing, indeed, which appeared to give him pleasure and animation, was to speak of his views in regard to the penetration of the spermatozoon within the zona pellucida having been confirmed by recent observers. He said he had been patiently "*biding his time*" in full certainty that such would be his eventual reward, and predicted that in this matter his formidable opponent Bischoff, would yet become his ally in belief. This, as we all know, has been the case. He also said, in alluding to a recent report of a committee of the Physiological Society (of Edinburgh), and in reply to our own objections urged against his ideas respecting muscular tissue, that he was as convinced of his correctness in reference to the double spiral structure of muscular fibre, as he ever was of the penetration of spermatozoa within the zona pellucida of the mammalian ovum.

He removed to Beccles in Suffolk, during the summer or autumn of 1853, and took up a residence near his brother-in-law, Dr Dashwood (who married Dr Barry's own and only sister), and remained here until his decease on the 27th of April last. Notwithstanding the continuance of his severe neuralgic affection, and otherwise gradually declining health, Dr Barry still devoted most of his available time to preparing a series of papers which were intended to prove that some of the views in regard to the *cell nucleus*, *fibre*, and *penetration of the zona pellucida by spermatozoa*, which he had propounded years ago, were receiving confirmation and countenance, as established doctrines in physiology. These papers were published in the "*Monthly Journal of Medicine*," as follows:—"Remarks on Quekett's Histology; on Kolliker's Human Histology, Vol. I.; and on the Physiological Importance of the Nucleus of the Cell," (June and July 1854). "Confirmation in two quarters of the Discovery by Keber of the Penetration of a Remarkable Body, believed by him to be a spermatozoon, into the Ovum of a Fresh Water Mussel." (January 1855). "Meissner shown to have been the first who confirmed the fact that the spermatozoon penetrates into the interior of the Ovum of the rabbit, the animal in which such penetration was first observed." (February 1855.) "Postscript to a paper in the January number of this journal, confirming the discovery by Keber of a Remarkable Body penetrating into the Ovum of the Fresh Water Mussel." (April 1855).

We find from a remark of his own appended to the last named paper, that so late as March (the month before his death), he was engaged in microscopical study. We further learn from a near relative that "the last few months of his life were employed in a review of his microscopic observations, and in forming, at the request of foreign physiologists, an abstract of them which he sent to Germany. It is remarkable that some portion of this paper occupied his last hours, and he appeared to have a satisfaction in having done with it, as one leaving the world." His perceptible decline seemed hastened by a circumstance explained in the following extract from a letter written on the 29th March, to a friend in Edinburgh,—"This is just a line from a sick-bed, in which I am imprisoned, the immediate cause being an ill-fitting boot. In my state of extreme prostration, the wound has refused to heal. A sinus formed which had to be laid open, and at one time I was by no means sure that the knife would not have to be followed by the saw." From another letter penned about a week before his decease, he appears to have rallied in some degree, and gives special directions concerning the transmission southward of his valuable collection of eggs. For some little time before his death he was exempted from suffering, and probably employed part of this period of relief in concluding the paper before referred to. He was fully aware of his approaching end, saying he had done with his "idols," and had attained "more than a willingness to cast them 'to the moles and to the bats.'" In the immediate prospect of death, he said, "All is peace;" on its being asked,

"Even now!" he said, "Even now;" and soon afterwards quietly passed away full of a Christian's hope.

J. B.

In enumerating the more important of Dr Barry's writings, we avoided any critical or other observations thereon, because we are able to place before our readers the following remarks which have been kindly supplied to the Editor by Professor Allen Thomson, who from long-continued and trustworthy researches in embryology, and complete acquaintance with the literature of this subject, is specially qualified for estimating Dr Barry's position as a physiologist:—

11*th May* 1855.

My Dear Sir,—Your letter has brought me the first intelligence of Dr Martin Barry's death. Along with all those who knew Dr Barry's amiable disposition, and had the pleasure of his friendship, I feel deeply grieved by his loss. No one who is acquainted with those departments of physiology to which he particularly devoted his attention, can doubt that in him the science has been deprived of one of its most ardent and valuable cultivators.

It would be difficult in a short space to present a just estimate of the value of Dr Barry's researches. It has appeared to me, indeed, that he himself did not appreciate rightly the relative importance of several of his views and observations. A natural and laudable ambition to be regarded as a discoverer, led Dr Barry to attach to some of his investigations, which he conceived were the most original, greater importance than will probably be accorded to them by the general consent of physiologists; while others of his observations will, from their minuteness and accuracy, probably retain their value, apart from the speculative views with which he connected them.

Physiology owes to Dr Barry two important discoveries: the first, that of the segmentation of the yolk in the mammiferous ovum; the second, the penetration of the spermatozoa within the zona pellucida or outer membrane of the mammiferous ovum.

As a student in Edinburgh, Dr Barry was known for his ability and research, and for the minuteness and accuracy of his observations and dissections. I am not aware of the exact period at which his attention was first directed to embryological pursuits; I think it must have been about or soon after the year 1835. Shortly after that period he showed his acquaintance with the subject by a translation of the first part of "Valentin's Manual of the History of Development," in the *Edinburgh Medical and Surgical Journal* for 1836, No. 127.

It was after this that he entered upon a comprehensive and well-conducted series of experiments and observations on the formation and earlier stages of development of the ovum in the rabbit, dog, and a variety of other vertebrate animals. The results of these researches were communicated to the Royal Society of London in three successive memoirs, which are distinguished for the extent and variety of the objects brought under investigation, the minuteness of the observations, the great beauty of the drawings representing them, and the generally correct views of the doctrines of embryology contained in them. It is not for me here to enter upon any analysis or criticism of these memoirs, or still less to discuss any of those parts of Dr Barry's researches, the accuracy of which has been called in question by other observers. It is enough for me at present to say, that, whatever difference of opinion might exist as to some points, all embryologists of note have long ago concurred in thinking that they contain a large amount of valuable original observation, and of materials for the elucidation of the subject of which they treat; and it is now also agreed by most that the two discoveries to which I have referred, and which are contained in these memoirs, belong to Dr Martin Barry.

In the description which Dr Barry gave of the segmentation of the yolk of the mammiferous ovum, obtained from the Fallopian tube (see second memoir,

Phil. Trans. for 1839), it may be that his interpretation of the nature of this remarkable phenomenon is somewhat different from that which most physiologists, with the extended and improved observation since made of the corresponding phenomena in the ova of a great many other animals, may now be disposed to adopt; but we ought not, on this account, to deny to Dr Barry the merit which is unquestionably due to him, of having first extended to mammalia, the observation of a phenomenon which had previously been known only in the Batrachia, and of thus giving a remarkable impulse to the observation of the earliest changes of the yolk succeeding to fecundation.

With respect to the second discovery to which I have referred, it is well known that much doubt for a long time prevailed. In the third memoir, read to the Royal Society in May 1840, Dr Barry stated, that he had observed a small body like a spermatozoon passing into the ovum of the rabbit, through an aperture or cleft in the zona pellucida. But as this was in itself a novel observation, and had been made on an ovum taken from the Graafian follicle of the ovary, it did not obtain much credence. In October 1843, Dr Barry published a notice of an observation, which he had then made, of spermatozoa within the ovum of the rabbit taken from the Fallopian tube, and in process of segmentation. In this instance, although there was no aperture to be perceived in the zona, as in the former instance, Dr Barry conceived his first statement to be fully confirmed. It is familiar to most of the readers of physiological works, that even this last observation, though much more articulate than the first, was met with a very positive denial by Bischoff and others, who failed to confirm the fact of the penetration of the spermatozoa into the ovum; and that it was not till nine years afterwards, that the observations of Dr Nelson on the impregnation of the Ascaris mystax gave a new impulse to the study of this subject, and that Mr Newport was led to the discovery of the penetration of the spermatozoa into the ovum of the frog; and that still more recently the careful re-examination of the subject in mammalia, has resulted in the complete confirmation by Bischoff himself, Meissner, and others, of the fact of the entrance of spermatozoa into the mammiferous ovum, as stated by Dr Barry in 1840, and, more particularly described and figured by him in 1843.

Knowing the ardent desire which Dr Barry always cherished to rank among original physiological discoverers, we can readily conceive and sympathise with the lively satisfaction he must have felt at finding not only his single observation confirmed, but the fact which it attested in one important class of animals extended to a number of others, and thus made, in some measure, the foundation of a great general law. Nor can we be surprised that, to a mind so sensitive as Dr Barry's, the doubt, and almost obloquy, thrown over his observations by the disbelief in his statement as to this and some other points, should have been converted into exultation, in witnessing the discomfiture of those who, somewhat inconsiderately and intemperately, undervalued his researches.

The most valuable part of Dr Barry's embryological researches, besides the facts already adverted to, were those relating to the first formation of the ovum, and its earliest changes. He was not so fortunate by any means in his observations on the first formation of the embryo; but, indeed, he can scarcely be said to have entered fully into the investigation of this part of the subject. In so extensive a field of research, it might be expected that his views should have sometimes undergone a change in the progress of his investigations, and that he might also fall into errors to be corrected by subsequent observers.

Dr Barry's embryological researches very soon led him also into histological observations. Together with all those who followed the progress of anatomico-physiological discovery between the years 1835 and 1840, he was strongly impressed with the novelty and importance of the facts and views brought forward by Schleiden and Schwann on cytogenesis, and the relation of the process of cell formation and change to the origin and growth of the various textures of living bodies; and he already showed how fully he was

aware of the value of these discoveries, in their application to embryological research, in an appendix to the third of his memoirs on that subject.

He also went much farther in this class of observations, and conceived that he had been fortunate enough to arrive at new and important conclusions with respect to the nature and functions of the cell nucleus, and the intimate structure of the muscular and other fibres of the animal body.

In the end of 1840, and in 1841, Dr Barry presented to the Royal Society three memoirs on the corpuscles of the blood, in which, besides a considerable amount of observations on these elementary structures, he brought forward new and speculative views as to their origin and relations to the other organised parts of the body.

He endeavoured to show that the blood corpuscles are derived from or are the descendants of the nucleus of the original germ-cell, and of its progeny of cells, that they are capable of multiplication by division of their nuclei in the endogenous manner, and that they, the blood corpuscles, are the source of all the organised textural elements and formed textures of the animal body.

In the end of the same year (31 December 1841), Dr Barry presented to the Royal Society another histological memoir, entitled, "On Fibre," but which, as he himself states, was intended rather as a continuation of those on the corpuscles of the blood, than as the history of a distinct series of investigations: and it was in this memoir that he first brought forward as the result of his microscopic observations, those views as to the spiral structure of the muscular fibre, the blood corpuscle, and, indeed, all the textural elements of organised bodies, which engaged the greatest share of his attention during the latter years of his life, and on which he seemed more disposed to rest his character as a scientific observer and discoverer than on any other part of his labours.

The whole of Dr Barry's memoirs may be said to be pervaded by a tendency to speculation, and by a fixed determination to bring all his observations under a novel and philosophical system of doctrine; this tendency seems to have become stronger in the course of his researches, and to have shown itself more and more apparently as his attention was more confined in the observation of the organic cell and its nucleus. Attracted by the apparent universality of the laws of cellular growth which had been divulged by Schleiden and Schwann, he had already conceived the fanciful and the important discovery that the first rudiment of the body of the embryo proceeds directly from the linear extension of the nucleus of one of the cells descended from his "germ-cell," a view totally unsupported by, and at variance with, all other observations on the same subject. And there can be little doubt that, as he proceeded with his histological researches, the speculative views gained far too great a preponderance over the objective or mere matter of fact character of the results to be deduced from his observations.

After a continued, close application to these microscopic researches, from 1837 to 1843, Dr Barry fell into bad health, and his sight was so much injured, that he was obliged, even after the partial recovery of his general health, for several years altogether to abstain from the use of the microscope. It may reasonably be supposed that, with a mind so active as Dr Barry's, and so intent upon founding a new histological system, the circumstance must have had considerable effect in giving preponderance in his latest papers to the speculative element.

While Dr Barry adopted in a great measure the views of Schleiden and Schwann as to the cellular origin of the textures, he conceived that he had made an important addition to, and correction of, their views, in calling attention to the activity of the nucleus of the cell as a productive agent. Adopting a term suggested by Professor Owen, he denominated the substance of the nucleus "hyaline," and he came to the belief that in every species of growth or production, it is by descent from, or by division of, a portion of this hyaline-nucleus that the new parts first take their origin: and that in this

manner every part of an animal is directly descended from the hyaline-nucleus of the germ-cell. He also conceived that he had demonstrated that each nucleus possesses a minute orifice, corresponding apparently to the nucleolus, which he regarded as the absorbent aperture through which the materials necessary for the further changes in the nucleus are introduced. A collected account of these views was read to the Wernerian Natural History Society of Edinburgh, and published in Jameson's *New Phil. Journal* for 1847.

The histological views adopted by Dr Barry in the three memoirs to which I have last referred, viz., in his papers on the "Blood Corpuscles," "On Fibre," and on the "Nucleus of the Animal and Vegetable Cell," did not meet with that assent from the opinion, nor support from the observations of other physiologists which he had fondly anticipated; and they were even somewhat roughly handled by some writers on histology. The confidence of their author in their truth did not appear from this, however, to have been in any degree diminished, and, accordingly, after a partial recovery of his health and his eyesight, we find him interesting Professor Purkinje of Breslau in his views, to such an extent as to induce him to repeat with him a number of his observations on the spiral structure of muscle, and some other textures, and to translate into German a long paper of Dr Barry's on that subject, which was published in Muller's *Archiv. of Physiology* for 1850.

An English version of the same paper, somewhat abridged, together with newer observations of his own, and deductions from those of others, was also published by Dr Barry in the *Philosophical Magazine* for August 1852.

Shortly after that time, I received several visits from Dr Barry, during which we made together, some microscopic examinations of specimens of muscular fibrillæ, I had in my possession. Knowing well Dr Barry's desire to obtain assent to his views, I took pains, both in conversation and in writing to him afterwards, to assure Dr Barry that I was not a convert to his views of the spiral structure of muscular fibre; but I found that his anxiety to obtain support, led him to attach weight to every expression, however indirect, which seemed to indicate assent, while much stronger expressions of dissent were disregarded. The result of these and other examinations by Dr Barry, were recorded in a second paper on muscular fibre, published by him in the *Philosophical Magazine* for November 1853.

It would take a much more detailed consideration of the whole series of observations recorded in Dr Barry's papers, than I could enter upon here, to enable me to give any criticism of these researches, or to explain the circumstances in which I think he may be in the right or have fallen into error. Now that he is no more, and his works alone remain, it will be more pleasing to endeavour to extract that which is good and true from them, rather than to discuss fully their errors, and this will be, I hope, the result of continued inquiry. In the meantime, if it may be permitted, in these circumstances, to make only one remark on the general character of Dr Barry's histological researches, I would say that, while they present us with most commendable examples of literary research, scientific acumen, and patient, minute, and persevering observation; they also show the injurious effect which the predominance of too speculative a view in the mind of an observer, may exercise on the observation and description of the appearances which form the subject of his investigation.

I esteemed Dr Barry as a friend, and valued him as a most ardent and persevering inquirer. Whatever may be thought of his individual observations, it cannot be doubted that his researches as a whole, gave a decided impulse to the progress of knowledge in the departments of which they treat, partly by the actual contribution of some new and valuable facts and observations, and partly by the ingenuity of his speculative views, the vigour with which they were supported, and the discussions to which they necessarily gave rise. And while we must regret the loss of one so well qualified to engage in scientific research, we may justly place Dr Barry in the rank to which his ambition

aspired, that, viz., of a meritorious and successful scientific inquirer; for, assuredly, the reputation of all these should be held in grateful remembrance, who, besides having themselves added several important facts to the subjects of their investigation, have also, by the influence of their example, the extent of their labours, and the ingenuity and vigour of their writings, promoted the advancement of the science of organization. I must beg you to excuse the appearance of haste in these observations, which in the midst of many other engagements, I have, at your request, thrown together.—I am, &c.,

A. T.

On the Horizontal Curvature of the Internal Femoral Condyle: on the Movements and Relations of the Patella; Semilunar Cartilages; and Synovial Pads of the Human Knee Joint. By John Goodsir, F.R.S.S. L. and E., Professor of Anatomy.

(*A Lecture delivered in the University of Edinburgh.*)

The lectures, consisting the ordinary descriptions of the human knee-joint, and the more precise observations of the Brothers Weber (*Mechanik der Menschlichen Gehwerkzeuge*, 1836, as presenting the present state of information on the subject; proceeded to explain the arrangement and use of the peculiar curvature at the fore part of the inner condyle of the femur, as recently determined by Professor Meyer of Zurich (*Die Mechanik des Kniegelenks, Müller's Archives*, 1852); and the movements and relations of the patella, semilunar cartilages, and synovial pads of the articulation, as observed by himself.

Before entering on the peculiarities of the inner condyle, Mr G. reminded his audience that the knee-joint consists of two articulations, with a common synovial membrane, the patello-femoral, and the femoro-tibial, the latter being double. The articular surface of the femur is consequently divided into a trochlea for the former, and two condyles for the latter; the condyles being separated from one another by the intercondyloid fossa, and from the trochlea by two shallow oblique grooves.

Mr G. now observed, that it appears to have not hitherto noticed that the so-called obliquity of the inner condyle of the femur is in fact, as Meyer has pointed out, a curvature of its anterior third, with its concavity directed backwards, outwards, and downwards. The two posterior thirds of the inner condyle pass backwards parallel to, and have the same general form and extent as the entire outer condyle of the bone. The curved portion, or anterior third of the usually so-called inner condyle, may therefore be conceived as a part interculated between the patellar trochlea and the proper inner condyle.

According to Meyer, the mechanical advantages, which result from the peculiar antero-posterior curvature of the femoral condyles, which the Brothers Weber concluded, from their measurements, to be spirals, may be with greater simplicity assumed to consist, as his own measurements suggest, each of two circular segments, the posterior of 120, the anterior of 40, the radius of the former being to that of the latter as 5 to 9. The horizontal curvature at the fore part of the inner condyle, or, more precisely, the oblique curvature may, as Meyer states, be conceived as a segment of 60° of the margin of the base of a cone, the axis of which is directed at an angle of 45° downwards, outwards, and backwards, in front of the spine of the tibia, so that its apex is situated in the external condyle of that bone.

In flexion and extension, therefore, of the knee-joint, as long as these movements are confined to the outer condyle, and the two posterior thirds of the inner condyle of the femur with the condyles of the tibia, they take place round two transverse axes, which pass respectively through the centres of the posterior antero-posterior circular curvatures of the femoral condyles, and the centres of the anterior. To simplify the conception, however, of this part of the arrangement, Meyer assumes as sufficiently accurate, a single transverse axis.

In the first third of flexion, and in the latter third of extension, the movements of the femur and tibia take place round the oblique curvature or anterior third of the internal femoral condyle; and involve, in addition to the completion and commencement of flexion and extension respectively, a movement of rotation of the tibia, and, consequently, of the leg and foot inwards in the former, and outwards in the latter. These remarkable movements of rotation inwards and outwards, inseparable from the commencement of flexion and the completion of extension, take place round the axis of the ideal cone already alluded to. This axis Meyer denominates the oblique axis of the knee-joint.

These movements of rotation, combined with flexion and extension, must be carefully distinguished from those of which the joint is capable when considerably flexed. The latter, with which anatomists are already familiar, take place, in general terms, round a prolongation of the axis of the tibia. This axis Meyer denominates the rotation axis of the knee-joint.

Mr G. next referred shortly to the action of the ligaments in the movements round the oblique axis of the joint. Of these, the most remarkable is that of the external crucial, which ligament becomes tightened in extension, as the movement round the oblique curvature of the inner condyle proceeds, and thus acts, from its obliquity, in a direction from below, upwards, backwards, and outwards, so as to guide the rotation of the tibia outwards.

The discovery of the oblique axis of the knee-joint has enabled Meyer to determine with greater precision the action of certain muscles of the thigh. The use of the peculiar mode of insertion, hitherto unexplained, of the sartorius, gracilis, and semitendinosus, becomes evident. Their tendons passing down behind the inner side of the knee, curve forwards and outwards on the tibia; so that these muscles effect that rotation inwards, which is a necessary accompaniment of the commencement of flexion. These muscles produce this rotation directly; that is, by an adaptation of their tendons to the purpose; but, according to Meyer, the proper flexors of the knee, the biceps and semimembranosus, only act indirectly as rotators, through the medium of the articular surfaces and ligaments. Mr G., however, conceives that the latter may act directly in producing rotation inwards at the commencement of flexion, for its tendon, instead of being inserted, as is usually stated, into the back part of the inner tibial condyle, passes forwards and outwards round the head of the bone in a distinct grove, in which it moves, being kept in its place by prolongations of the internal lateral ligaments of the joint; and thus presenting the same general mode of insertion as the three muscles already alluded to.

The rotation outwards, at the completion of extension, is produced indirectly by the quadriceps-extensor; the form of the articular surfaces, and, according to Mr G., the tightening of the external crucial ligament, co-operating with the group of extensor muscles.

Meyer has detected a very beautiful adaptation of parts in connection with this latter movement, and has thus explained the characteristic enlargement, and the extensive attachment to the patella, of the vastus internus muscle. When the knee is extended, the ligamentum patellæ, instead of being perpendicular, will be found to pass downwards and outwards to its tibial attachment, which has moved outwards in the rotation of the leg. The lower portion of the vastus internus is enlarged, and the upper portion of its tendon is attached to the greater part of the inner edge of the patella, for the purpose of preventing that bone from being pressed against the outer part of the femoral trochlea during the rotation outwards of the leg, by drawing it inwards and upwards, and keeping its axis in the line of the ligamentum patellæ; while the lower portion of its tendon passes down to be attached somewhat obliquely to the inner side of the head of the tibia, and thus assists directly in rotating the leg outwards.

Meyer has also shown that, in standing quietly upright on one or both limbs,